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PSYCHOLOGY AS A QUANTITATIVE RATIONAL SCIENCE[1]

By Professor L. L. THURSTONE
THE UNIVERSITY OF CHICAGO

THE purposes of this society are not new, but they represent an emphasis and direction which have not hitherto received major consideration in psychological science. It seems proper that we should devote some share of our first program meeting to a consideration of our main objectives.

Our main purpose is briefly stated in the subtitle of the new journal, *Psychometrika,* namely, to encourage the development of psychology as a quantitative rational science. More briefly, this may be called mathematical psychology. We should justify our emphasis upon quantification and upon rationalization in science, as well as our conception of the fundamental nature of science.

I assume that we are in complete agreement that we can not suddenly quantify our comprehension of psychological phenomena over their entire range. As psychologists, we are as interested as ever in making exploratory studies of new psychological effects and in discovering hitherto unknown effects. At present, the range of psychological phenomena that can be profitably reduced to mathematical formulation is limited, and it is likely that every man who works on a problem of mathematical psychology will also concern himself with exploratory studies of other problems that are as yet too new for detailed rationalization.

After the discovery of a psychological effect, we naturally turn to the second phase of scientific inquiry, namely, to relate the new effect in a simple descriptive manner to what is already known. In this stage, theories are devised to explain the experimentally known effects, and we try, of course, to make psychological theories less complicated than the effects that are to be explained. In this phase the descriptions of psycho-

[1] Abstract of address by the retiring president of the Psychometric Society at Hanover, N. H., September 4, 1936.

logical effects are quantified but seldom rationalized. Rough indices of covariation are in general sufficient. It is not often crucial that certain particular quantities are observed; it is enough to know that one sort of thing is larger than some other, or that one sort of thing tends to be directly or inversely related to something else.

A later phase is that in which theories in a given field are unified in terms of a relatively small number of postulates whose consequences can be deduced by logical considerations. The resulting formulations are submitted to experimental test with the requirement that the functions and the parameters produce meaningful results. This phase of inquiry is much more demanding than the generally descriptive type of work, and consequently many more failures of experimental verification are to be expected.

The mathematical and rigorous treatment of a psychological problem is in no sense a substitute for the exploratory and descriptive types of experimentation. In general, mathematical formulation of a problem can only follow upon the exploratory and descriptive studies by which we first become familiar with the phenomena that call for reduction in more rigorous analysis. The history of science shows exceptions where analytical formulation far preceded experimental knowledge, but these exceptions are not a safe guide for most of us. Most of us will be the more useful by staying close to experimental check. The flights of analytical genius are attended with a risk of error that is proportionate to separation from the laboratory.

Let us mark, then, three differentiable stages of scientific inquiry—exploration, descriptive relation and rationalization. It is to be noted that every science has problems in each stage of development. No science can be characterized as entirely of one type. In a young science like ours, it is to be expected that most of our work is in the first two phases, but it is encouraging to find that some of our problems can be pushed toward more rigorous treatment.

For the past twelve years I have been trying to introduce quantitative rational theory into psychology. Instead of merely discussing this subject in general terms, I shall describe some attempts to put these ideas into practice.

In building a unifying rational structure for psychophysics, I began by considering a simple table showing the frequency with which each of several stimuli is judged to be stronger than each other stimulus. The first question was then, to ascertain whether such a table reveals some underlying order or law. I found a few simple postulates that would lock the whole table of experimentally determined proportions. These included a new concept in psychophysics, namely, the discriminal error. The result was the law of comparative judgment. Armed with this law, we can investigate the relations of the several psychophysical methods. For example, the simple method of rank order can be theoretically related to the constant method so that one set of experimental results can be predicted from the other.

An interesting result is that we can ask new questions which we had not thought of before. If the discriminal error is an attribute of each stimulus, what should be the consequences when this value is larger than in the conventional limen determinations? If the analysis is sound, then the so-called psychometric curve should be positively skewed for coarse discrimination. Experimental results on brightness discrimination of rats verified the theory. The new psychophysical concepts are applicable to the measurement of affective values, such as in esthetics, and to many other forms of cognitive and affective discrimination. These concepts and laws are quantitatively formulated and they can be tested experimentally. Psychophysical experimentation is no longer limited to those stimuli whose physical magnitudes can be objectively measured.

One of the best possibilities for the development of rational theory in psychology seems to be in the study of learning. It is a matter of very general observation that in certain forms of rote learning the errors are gradually eliminated until a perfect performance is attained. In the descriptive theories of learning occurs the idea that the making of an error has the effect of reducing the probability of its recurrence, and that a successful act tends somehow to augment the probability of its recurrence. In a first approximation to the rationalization of this problem, I assumed that the extent to which we profit by our mistakes is equal to the extent to which we profit by our successes. When these relatively simple psychological ideas are stated symbolically, they take the form of a differential equation whose solution gives a rational equation for the rote-learning function.

Instead of merely fitting a hyperbola with arbitrary parameters to experimentally determined learning records, as I did when I was a student, we are now dealing with an equation whose parameters have meaning in terms of the psychological postulates that the equation represents. The logical manipulation of these postulates leads to some interesting conclusions which must be tested experimentally. Some of these inferences are that the curve for a long rote-learning task should first be positively accelerated and later negatively accelerated. When the task is heterogeneous, the inflection point of the curve should be lower than when the task is homogeneous as to the dispersion of difficulty of its elements. A list of about fifteen such

ferences can be drawn from the postulates and each them can be tested experimentally. Mr. Gulliksen ecame interested in this problem and he has investiated logically the consequences of assuming differenal rates at which we profit from success and from rror. These hypotheses lead to a different set of nferences which are also subject to experimental heck.

When we set down a few psychological postulates n the form of a differential equation, the equation is ometimes looked upon as something mathematical, and herefore non-psychological. The equation does not epresent a postulate in mathematics. It states a psyhological postulate in the concise and universal lanuage of science, namely, mathematics. If the reader oes not understand it, he has the same handicap as f the postulate were stated in Russian or in some other anguage which he can not read. The equation is the tatement of a psychological idea.

Every rational formulation of a scientific problem s a challenge to extend its range of application. It hould be examined to see what happens when the parameters take limiting values. If the formulation does not make sense at the limiting values, our faith in it must be restricted. By making approximations and adaptations, I arrived at the conclusion that there should be a relatively simple relation between the length of learning time for rote material and the number of items in the lists to be memorized. In fact, the learning time should vary approximately as the 3/2 power of the number of items in the list, irrespective of whether the items are numbers or nonsense syllables. This was an intriguing possibility and, before setting up separate experiments, I searched for available published data. I found nine experimental studies with data for this problem, including the very complete study by Lyons. In all nine of these studies the 3/2 power law was verified. Some of the subjects learned faster than others, and some of the material was intrinsically more difficult than other material, but all the experiments on rote learning verified the 3/2 power law. This is a psychological uniformity that transcends the particular numerical results in each experiment.

A study has recently come to my attention which is encouraging in the application of quantitative rational theory in the field of animal learning. Mr. and Mrs. L. E. Wiley have applied a rational learning function to the learning records of rats, with the special problem of ascertaining whether each animal can be described in terms of a learning constant for a certain type of maze. The mazes are represented in the experiments in five degrees of difficulty. The experiments were done under the general supervision of Professor Lashley. I have examined the preliminary results and I find them to be surprisingly close to the rational learning function.

There is one field of psychology in which quantification has been the rule for many years, and which should therefore have some appraisal in an outline of the objectives of this society. I am referring to the field of psychological and educational tests in which quantitative and statistical procedures have been taken for granted for several decades. No student of psychology can make even a superficial review of the literature in this field without some elementary knowledge of statistical theory. If we take stock of mental testing in relation to the psychological profession as a whole, I believe we must admit that mental testing does not have so much prestige as some other types of psychological work. There is probably good reason for this circumstance. The majority of students in this field are not expected to master the analytical theory of their subject. Too frequently they learn merely the routine of giving various tests, and college credit is given for memorizing this routine. Perhaps it is significant that there is not a single text-book on the theory of psychological tests, although the book market is full of manuals that limit themselves to the description of tests, the routine for giving them, and verbalized interpretations. With increasing attention to the quantitative logic of psychological measurement, it is quite likely that mental testing will be more favorably regarded among psychologists.

One of the old psychological measurement problems is the quantitative description of mental growth. To draw a physical growth curve for stature or for weight is a relatively simple matter when compared with the difficulties of drawing a mental growth curve. In the physical measurement, we have a unit of measurement in the centimeter scale. The application of calipers and tape to each individual is little more than careful routine. But in the measurement of mental growth there was no yardstick previously available and there was no mental unit of measurement. The mental age, or the score in an intelligence test, is logically no more than a tag by which the individuals in a group can be arranged in an ordered series, but that is not measurement. To say that A, B and C have scores or mental ages of 5, 6 and 7, respectively, does not say anything about the relation between the increment A-B and the increment B-C. To say that a person's score on a mental test is zero does not justify the conclusion that he is of zero intellect. Before the mental growth curve could be drawn, the problem of measuring intellect had to be solved.

An ordered series can be transformed into measurement if the frequency function is known. It has been customary to make this transformation on the assumption that a random sample of children has a Gaussian

distribution of intellect at point age, but this assumption has often been questioned because it has not been critically tested until recently. By making this assumption and by allowing only two parameters for each age, to represent mean and dispersion, it was found that there was a linear relation between the absolute variability and the mean test performance for successive age groups. The appearance of this relation led naturally to the definition of an absolute zero of intellect as the value of the mean test performance when variability vanishes. In the nature of the case, variability can not be negative. With these developments and other critical studies of the underlying logic of mental measurement, it is now possible to draw legitimate mental growth functions for the various tests in current use.

Since most available data on psychological tests refer to children of ages five or six and older, there was necessarily a long extrapolation to locate an absolute zero. I have studied similar data for a group of children who were examined monthly or quarterly from birth during the first three years of life. The absolute scaling of these data shows internal consistency. It is of interest to note that the mental growth curve for infants is positively accelerated and that similar studies on older children show the same effect until about eight or nine years of age, after which the mental growth curve is negatively accelerated toward an adult asymptote. One of the most interesting findings concerns the age at which the mental growth curve starts at absolute zero. If we assume that the mental growth curve is continuous below the first month, as it is after that age, then the growth curve starts at absolute zero about four or five months before birth. I have been assured that such a psychological finding is consistent with the normal neurological development of the fetus.

The analytical work of discovering the nature of the abilities involved in psychological tests has been done by Cattell, Thorndike, Spearman, Kelley and by many others who are turning their interests to these problems. The recent extension of Spearman's single factor analysis into any number of dimensions has given a new leverage on this problem, and within a few years we shall have a much better knowledge about the nature of the abilities that can be appraised by means of psychological tests.

Factor analysis is a method of scientific exploration that is not limited to the psychological problem of finding primary abilities. The factorial methods are general, and they can be applied to many different problems that are logically similar. We start with a population of entities that may be individuals, geographic areas, occasions in time or anything else that can constitute the members of a population in the sense of this term in statistical theory. Let each member be known in terms of n attributes. These attributes may be complex and conventional, and they may be overlapping to an unknown extent. It is assumed that we can ascertain the degree of covariation of these attributes. By this we mean that those members who have one attribute to a marked extent may tend to have a certain other attribute also, or the relation may be inverse so that those who have one attribute tend to be noticeably deficient in another attribute. The intercorrelations of the attributes constitute the experimental record of the degree of relation between the attributes.

Given a system of more or less related attributes, the factor problem is to ascertain whether there is some underlying order among them whereby we may be able to comprehend all the n attributes in terms of a smaller number of more fundamental or basic attributes. If such an order can be discovered, and if it can be shown to be unique, then we shall have a very special interest in ascertaining the nature of the smaller number of primary or basic characters which combine to form the larger number of attributes of the system as it is known directly in experience. As scientists, we have the faith that the abilities and personalities of people are not so complex as the total enumeration of attributes that can be listed. We believe that these traits are made up of a smaller number of primary factors or elements that combine in various ways to make a long list of traits. It is our ambition to find some of these elementary abilities and traits. In the mathematical analysis of the problem, it can be shown that the number of basic factors is the minimum rank of the correlational matrix with unknown self-correlations.

All scientific work has this in common, that we try to comprehend nature in the most parsimonious manner. An explanation of a set of phenomena or of a set of experimental observations gains acceptance only in so far as it gives us intellectual control or comprehension of a relatively wide variety of phenomena in terms of a limited number of concepts. The principle of parsimony is intuitive for any one who has even slight aptitude for science. The fundamental motivation of science is the craving for the simplest possible comprehension of nature, and it finds satisfaction in the discovery of the simplifying uniformities that we call scientific laws.

Mathematics is sometimes regarded as a tool or method that can be applied to psychological problems. It is regarded somehow as coordinate with other methods, such as the experimental method and the comparative method. This is just as sensible as it would be to say that theoretical or systematic psychology is a method. The mathematical statement of psychological

postulates and the symbolic representation of fundamental concepts in our science is the natural development of systematic or theoretical psychology which concerns itself with the foundations of our science. In testing psychological theories, stated in quantitative form, we may resort to various experimental methods. But it is hardly correct to say that the invention and development of the theories themselves constitute a psychological method. We shall probably have in our field, as in physics, those who find their talent principally in the analytical study of fundamental concepts and theories, and others who find their best talent in the invention of ingenious experiments for testing psychological theory. The development of our science will probably follow the pattern of all science in becoming more and more mathematical as fundamental ideas become more rigorously formulated.

One of the most fruitful ideas that we can give students is that most of the functional relations in nature are continuous. There are exceptions here and there in which critical values appear with sudden discontinuities, but these are exceptions. Lack of faith in the continuity of natural phenomena is one of the principal handicaps of the student who has not acquired the intuitions of science. Our teaching methods are perhaps largely responsible. Instead of teaching students to compare the first, the second and the third trial of a performance, and the A-group and the B-group in some experiment, we should set up some of our teaching experiments so that the student learns to look for functions rather than discrete comparisons. I shall give a specific example. In studying color vision, the student learns that the maximum saturation of an induced color is produced when the gray value of the background is the same as the gray value of the inducing color. Let this be the object of an experiment with a fixed inducing color. Let the gray value of the background be varied systematically, and let the experimental result be plotted as a continuous curve. It then appears that the maximum saturation of the induced color occurs at the gray value which is equal to that of the inducing color. The student gets the idea that his experiment deals with a functional relation.

We should clarify the distinction between statistics and mathematical psychology. A study can be quantitative without being mathematical. Merely to count noses or the answers in a test or seconds of reaction time or volume of secretion does not make a study either mathematical or scientific. This is not unlike the confusion by which arithmetical labor is sometimes called mathematical. Statistical theory is a branch of logic, and as such it is applicable in all science. If the scientific rationalization of a problem demands a certain relation between parameters or that a certain function should be parabolic or that a certain parameter should have a predetermined value, and if the experimental data have been obtained for testing the hypothesis, then the statistical methods are useful in comparing the agreement between theory and experiment. This is, in fact, the principal usefulness of statistical theory. Hence, a knowledge of statistical theory should be part of the stock-in-trade of every experimental psychologist.

A crucial matter in the development of a psychological science is the training program for the students who are to build this science. The first requisite is some familiarity with basic science, both physical and biological. Without this familiarity the student can hardly be expected to help in building a new science. If he expects to participate in making psychology a quantitative and a rational science, he must know something about the language of science, namely, mathematics. The most profitable study of mathematics is probably done after it has become motivated by a realization that it does function, not merely as an aid or tool that a psychologist can use, but as the very language in which he thinks. It has been my experience that some students who are themselves unable to develop a mathematical idea are nevertheless well able to comprehend an essentially mathematical formulation of a psychological problem with its implications and experimental possibilities. Such a student may be more fertile with ideas than one who possesses considerable mathematical skill without the flexibility of mind that is essential in creative scientific work. More fortunate is the student who has all these aptitudes.

We turn now to what I regard as one of the most fundamental issues in psychological science because it concerns the possibility of such a science. We have considered the desirability of advancing as rapidly as possible, and over an increasing range, to the rationalization of mental phenomena. A rational structure for a psychological science will rest on postulates concerning fundamental psychological concepts. But what shall be the nature of these postulates? Are we willing to build a science on psychological postulates, or shall we demand that psychology develop as a branch of each one of several neighboring disciplines?

In searching for basic concepts for psychology, we must be careful lest we borrow terminology that has prestige and forget to pick up also the ideas that belong with it. This leads to a confident glibness with important-sounding words that is not backed by the understanding which the language is intended to convey. It is not unusual to hear the conversationally minded carry on fluently about relativity and the indeterminacy principle in relation to social and psychological matters even when they know nothing about

these words in their technical setting. When we use a physiological or neurological manner of talking about psychological phenomena, it is well also to take note whether we really have any physiological ideas that are relevant to our problem. We may be convinced that a phenomenon is mediated by a physiological mechanism, but unless we have some physiological hypothesis concerning it, we might as well use the more direct and less pretentious psychological language.

Some rationalizations are regarded as more basic than others. We should like to have a precise physiological description of just what happens when we recall a nonsense syllable in a learning experiment. We should like it even better if the chemical equations could be written that cover the essential effects of recalling the nonsense syllable. Better still would be a detailed account, in terms of mathematical physics, of the molecular and atomic forces that characterize the recall of the nonsense syllable. In this hierarchy, we should gladly supplement a psychological explanation with one that is more basic. It would be unfortunate if the development of any psychological idea should be restricted because of a compulsion to make it look like physiology or to make it look like sociology.

Psychological theory can be rigorous. There is an erroneous impression among psychologists, as well as among our academic neighbors, that psychological ideas are necessarily loose, verbal, subjective and unfit for the quantitative analytical treatment of science. This impression is not justified. It is not necessary for us to abandon psychological concepts if we introduce analytical rigor in dealing with these concepts.

In rationalizing several psychological problems, I have been content to build with psychological concepts and postulates, even though I believe that some of them will be rephrased eventually in terms of physiology or chemistry or physics. But it is my conviction that we shall progress better by frankly building in terms of psychological concepts than by merely adopting a terminology and a manner of work which are premature for many psychological problems. It is better to formulate the laws of learning in terms of psychological ideas, and to find them experimentally verified, than to wait until the phenomena of learning can be rationalized in neurological terms. It may be a long time before that happens, even though we have faith that it will happen eventually. It is better to formulate the law of comparative judgment in terms of the discriminal error, which is a psychological concept, than to wait until we shall understand, physiologically, what happens when we say that one vase is more beautiful than another or that one synonym is better than another or even that one gray is darker than another. Even the simplest sensory comparisons are far from rationalized in physiological terms. If we isolate the primary abilities at first psychologically, perhaps we shall aid in their ultimate identification in physiological terms. I should make the plea that we develop psychological science frankly with psychological concepts, except in those cases where physical, chemical or physiological formulations are available.

In encouraging students to help us build an integrated interpretation of mental phenomena on an experimental foundation, let us remember that a psychological theory is not good simply because it is cleverly mathematical, that an experiment is not good just because it involves ingenious apparatus and that statistics are merely the means for checking theory with experiment. In the long run we shall be judged in terms of the significance, the fruitfulness and the self-consistency of the psychological principles that we discover.

OBITUARY

EDWARD CURTIS FRANKLIN

WITHIN the period of a year, American science has lost three distinguished pioneers in chemical research: Arthur Amos Noyes, on June 3, 1936, Julius Stieglitz, on January 10, and now Edward Curtis Franklin, on February 13, 1937. While they differed widely in personal traits and in the manner in which they applied their talents to the furtherance of research, each, in his own way, left an indelible impression upon American chemistry. Noyes was not a natural experimenter, but he possessed a keen, analytical mind which enabled him to concentrate his attention upon problems that seemed to hold forth promise of important results; his greatest contribution, perhaps, was the organization of two research laboratories and the selection and encouragement of able young chemists. Stieglitz was more the academician of the continental type; his genius found outlet in initiating research in numerous directions, which researches he carried out extensively through collaboration with many graduate students. Franklin was more of a lone worker; he carried on research to satisfy his innate curiosity; he worked in close association with only a few collaborators and the success of these researches was in large measure due to Franklin's consummate experimental skill.

Noyes, Stieglitz and Franklin all began their scientific careers as organic chemists, they all became interested in physical chemistry and made notable contributions to it; Noyes never returned to organic chemistry,

Stieglitz always retained a primary interest in organic chemistry, while Franklin developed a special field and applied to its development his wide knowledge of physical, inorganic and organic chemistry.

Noyes and Stieglitz received much of their scientific inspiration in Germany, where they obtained their degrees—Noyes at Leipzig, in 1890, and Stieglitz at Berlin, in 1889; Franklin was essentially an American product; he was, one might say, a self-made investigator. While he spent a year abroad in 1890–91, there is nothing to indicate that his foreign experience left any great impression on his mind; he spent a year at Johns Hopkins in 1893–1894 and received his doctorate from that institution on the basis of organic research carried out under Remsen. There is some evidence of Remsen's influence in Franklin's later work, but it is not marked. While Noyes and Stieglitz entered upon active research at a very early age, Franklin, who was approximately five years their senior, did not enter upon an active research career until he had reached the age of 35. To understand this delay in Franklin's scientific development, one must be familiar with the background of his early life. In the light of that background, the surprising thing is not so much that he began his research activity so late in life but rather that he began it at all.

Edward Curtis Franklin was born on March 1, 1862, in the little town of Geary City, in Doniphan County, which is located in the northeastern corner of Kansas. He entered the University of Kansas, which had been founded twenty years earlier, at the rather late age of 22; his brother, Will, a year younger, had entered the university a year earlier, and it was largely through his brother's influence that Edward Franklin followed him to the university. Will Franklin became interested in physics and had a notable career as teacher and writer; Edward Franklin took up chemistry, led to this, probably, by his earlier contact with pharmacy. One trait of Franklin's, and one that he never lost, was evident in his student days at Kansas—his capacity for forming friendships. These friendships were lifelong; included among his early intimates were a number of men who later achieved distinction, such as William Allen White, the journalist, and Frederick Funston, one of America's greatest adventurers.

In those early days, of which we are now speaking, the youthful University of Kansas had already achieved a position of some distinction for its researches in biological fields; botany, entomology and, particularly, paleontology. Samuel W. Williston, who had come to Manhattan, Kansas, in early life, from New England, was one of the leaders in research at Kansas; his special field was paleontology. There is little doubt but that the research atmosphere at the University of Kansas in the 80's and 90's was largely due to Williston's influence. In the physical sciences, no active research was carried out at the University of Kansas in those days; this is not surprising when one considers how few were the productive investigators in America at that time. It is not remarkable, therefore, that Franklin, who entered college at an age when students are usually graduating, should have been long delayed in training himself to become an accomplished investigator.

Any one who was not at one of the mid-western universities during the last quarter of the nineteenth century can have no comprehension of the meagerness of the physical facilities. The writer well recalls how Franklin cut up the jackets of Liebig condensers for the purpose of constructing Dewar tubes. Glass, suitable for glass-blowing purposes, was unobtainable in this country; chemicals had to be ordered from Germany and, frequently, had to be made because of the length of time required in obtaining them from abroad. Of the equipment employed in the early work on liquid ammonia, a resistance box and a simple Kohlrausch bridge were the only things that were not homemade.

Franklin hit his stride as an investigator when he began the study of liquid ammonia solutions. The work on liquid ammonia was initiated at Kansas by H. P. Cady with Franklin's encouragement. Later, Franklin and the writer undertook a series of investigations dealing with the problem. The success of the work was due, primarily, to Franklin's superb manipulative skill, which made it possible to successfully carry out measurements with the limited facilities then available. It is interesting to note that many of the results of these early investigations of Franklin's with several collaborators remain to this day almost the only reliable data available. Nothing shows more clearly Franklin's remarkable ability as an investigator than the fact that he was able to plan and carry out physical measurements in a field that had scarcely been cultivated in America at that time and that the results of these measurements have stood the test of forty years.

Franklin was quick to grasp the significance of the results of the experiments with liquid ammonia. He soon saw that a striking parallelism existed between solutions in water and solutions in liquid ammonia. He pressed the analogy further, always devising experiments calculated to establish relations upon a basis of sound fact. He extended and applied his ideas to organic as well as inorganic substances, and with remarkable success. The whole he finally gathered together and published a few years ago in a monograph entitled "The Nitrogen System of Compounds."

As an experimenter, Franklin was unsurpassed. He constructed Dewar tubes as early as 1895, probably the first constructed in America, and he prepared

argon at about the same time and sealed it into Plücker tubes and examined its spectrum. In 1896, he constructed platinum target x-ray tubes to be used by Lucian Ira Blake in his lectures on x-rays; he was ever ready to help a colleague. Franklin enjoyed giving lectures, particularly, when they required difficult demonstrations. He never failed, for example, to prepare nitroglycerine in his lectures on organic chemistry and he delighted in showing what it would do. His lectures on liquid ammonia were something in the way of a work of art; he carried out most difficult reactions in sealed tubes which he carried about with him on his trips. He had the rare ability to present a subject interestingly as well as logically.

If there was one quality of Franklin's that stood out above all others, it was his power of making friends; where others made acquaintances, he made friends. He valued these friendships, and nothing gave him more pleasure than to take a trip through the country to meet again his friends of old or to make new ones.

Franklin was anything but a recluse; he loved social intercourse and gloried in the activities of life. His love of nature was second only to his love of science. Mountain climbing was his favorite recreation and there are few of the higher peaks of the Rockies and the Sierras that Franklin did not climb. In his later years, when climbing mountains was out of the question, his automobile became his out-of-doors friend and companion.

In Franklin were combined many rare qualities that endeared him to his friends; he could be frank without giving offense and he could criticize without provoking rancor, his fair-mindedness was as obvious as his lack of guile and he was generous to a fault. His friends will miss his ready smile and treasure the memories of their happy associations with him.

Vale!

CHARLES A. KRAUS

RECENT DEATHS AND MEMORIALS

DR. EDWARD S. ROBINSON, professor of psychology at Yale University, died on February 24 as the result of injuries received when struck by a bicycle. He was forty-four years old.

DR. RICHARD HOOPE CUNNINGHAM, neurologist and chief of clinic in the neurological department of the Vanderbilt Clinic of Columbia University, died on February 24 at the age of sixty-one years.

PROFESSOR E. S. ERB, for twenty-eight years a member of the department of agricultural chemistry at the Pennsylvania State College, died from a heart attack on February 19 at the age of fifty-nine years.

FREDERIC HEDGE KENNARD, associate in ornithology at the Museum of Comparative Zoology of Harvard University, died on February 24. He was in his seventy-second year.

RICHARD C. RADDATZ, since 1924 general assistant in the department of preparation of the American Museum of Natural History, an expert in mounting animals in habitat groups, died on February 21 in Nairobi, British East Africa. He was about fifty years old. Mr. Raddatz sailed from New York early in January with Mr. and Mrs. Philip M. Plant to collect specimens of wart hogs and ostriches for the Carl Akeley animal groups in the museum.

CRANDALL Z. ROSECRANS, assistant director and chief of the metallurgical division of the Leeds and Northrup Company, Philadelphia, died suddenly on January 7 at the age of forty years.

Nature reports the death of Professor Michael Lenhossék, emeritus professor of anatomy in the University of Budapest and president of the Hungarian Academy of Sciences, an authority on the histology of the nervous system, on January 26, aged seventy-three years, and of Dr. F. Sowerby Macaulay, known for his mathematical work, on February 9, aged seventy-four years.

THE following minute was drawn up by the Wistar Institute of Anatomy and Biology, Philadelphia, on the death of Effingham Buckley Morris: "From a long and active life in law, in finance and in the promotion of science Effingham B. Morris on January 22, 1937, passed forever from scores of devoted friends and admirers in all walks of life. To his associates in financial circles he was known for his integrity and vision in the management of affairs of magnitude; to those who knew him as a promoter of scientific knowledge he was admired for his resourceful efforts in building the institutions he administered; to those whose privilege it was to know him intimately he engendered a love and respect without limit. Members of the staff and employees of The Wistar Institute mourn the loss of a real friend; a member of the board of managers since 1915 and president of The Wistar Institute since 1922."

SCIENTIFIC EVENTS

THE COLUMBIA UNIVERSITY SCHOOL OF MEDICINE

GIFTS received by the Columbia University School of Medicine will enable it to advance its program of graduate medical education by enlarging its laboratory facilities at a cost of $500,000. Eight floors will be added to the west wing of the building of the School of Medicine at the Medical Center. The new construc-

ion is designed to house the graduate laboratories of the departments of anatomy, pathology, biological chemistry, physiology and bacteriology.

In discussing the plans of the school Dean Willard C. Rappleye said in part:

After full consideration of the great importance to the medical profession, the hospitals and the public of establishing graduate medical education at a high university level, the trustees of the university in 1932 created a higher degree to identify the individual who obtains that recognition as qualified by a university grade of training in one of the specialized fields of clinical medicine. A single level of graduate medical education is recognized for this purpose. Only residents appointed in one of the affiliated hospitals are eligible for registration for the degree of Doctor of Medical Science (Med.Sc.D.).

The new laboratories will provide the facilities for the necessary advanced work in the medical sciences. This work may be taken previous to the residency or be carried during the hospital period, if that can be arranged.

Affiliations with approximately twenty leading hospitals of the metropolitan area make available ample facilities for advanced clinical training and secure the participation in the program of a number of the outstanding clinicians of the vicinity.

The program conforms to the standards adopted in 1934 by the Council on Medical Education and Hospitals of the American Medical Association, and the Advisory Board for Medical Specialties, the latter representing the American Hospital Association, the Association of American Medical Colleges, the Federation of State Medical Boards of the United States, the National Board of Medical Examiners and the twelve national boards of specialists dealing with graduate medical education and certification.

Seven requirements for the advanced degree of doctor of medical science were listed by Dr. Rappleye. These are: graduation from a medical school approved by the university; completion of an internship of not less than one year in a hospital approved by the university; a three year period of study after the internship in the university or in approved hospitals and laboratories; intensive training in one or more of the basic medical sciences related to the special field of study selected; active experience during the three year period of not less than eighteen months in the hospital, clinics and diagnostic laboratories of the specialty selected; written, oral and practical examinations in the specialty elected and in related fields, and "an acceptable dissertation on an investigation conducted in or closely related to the specialty elected."

THE ELECTRONICS INSTITUTE AT THE UNIVERSITY OF MICHIGAN

AN Electronics Institute, consisting of a special lecture and conference program in electronics, will be held in Ann Arbor, as a part of the 1937 Summer Session of the University of Michigan, with the cooperation of members of the technical staffs of the General Electric Company, the Westinghouse Electric and Manufacturing Company and the Bell Telephone Laboratories.

The lectures will be given by Dr. Saul Dushman and Dr. Lewi Tonks, of the General Electric Research Laboratories; Dr. H. E. Mendenhall and Dr. F. B. Llewellyn, of the Bell Telephone Laboratories; Dr. Joseph Slepian and Dr. R. C. Mason, of the Westinghouse Research Laboratories; Professor Leonard B. Loeb, of the University of California, and Professor W. G. Dow, of the University of Michigan.

The program will consist of two independent four-weeks lecture sequences, dealing respectively with high-vacuum (June 28 to July 24) and gaseous-conduction electronic principles (July 26 to Aug. 20). In parallel problem laboratory and conference courses the lecture material will be worked into illustrative engineering problems, and teaching methods will be demonstrated and discussed. Opportunities for informal conferences will be provided. Courses in various cognate branches of electrical engineering, physics and mathematics will be included in the program.

The primary objective of the institute is to provide an opportunity for teachers and prospective teachers of electronics, engineers and physicists engaged in electronic development work in industry, and graduate students interested in electronics to broaden and unify their grasp of fundamental principles. It is believed that this opportunity for association between teachers and leaders in electronic research and development in industrial laboratories will help to clarify methods and policies in the teaching of the subject in engineering schools.

A special bulletin describing the details of the program is being prepared and can be obtained from Professor W. G. Dow, Electrical Engineering Department of the University of Michigan.

THE WISCONSIN ALUMNI RESEARCH FOUNDATION

THE University of Wisconsin Alumni Research Foundation, which was established in 1925, has appropriated the sum of $163,000 to the research funds of the university for the coming year for the support of both old and new research projects which are carried on under the direction of faculty members. All the projects, about eighty in number, are selected and approved by the University Research Committee, and the foundation which provides the funds has no voice in the selection or in the policies to be followed in carrying out the research work. This year's grant represents an increase of $20,500. It brings the total amount given by the foundation to research in the natural sciences during the last nine years to $83,033.

Of the total amount appropriated for the coming year, $100,000 is allotted to special grants-in-aid to stimulate university research. These are used to purchase equipment and supplies and to help to support

more than a hundred graduate research workers, thus enabling them to carry on their own research at the same time. Included in the grant again this year is an appropriation of $17,000 to permit faculty members to carry on during the summer certain lines of research which are already under way.

Funds are again included for the continuation of special fellowships and scholarships and for several post-doctorate fellowships. $20,000 is appropriated for the special fellowships and scholarships which were inaugurated two years ago. Known as the Wisconsin Alumni Foundation fellowships, these special fellowships and scholarships are granted to the most gifted young scholars and scientists that can be found in the United States.

The sum of $7,500 is included in this year's grant for the continuation of several post-doctorate fellowships with which it will be possible to bring to the state university unusually gifted men who have already proved their ability to carry on independent research work in the natural sciences.

A new item in the allotment is a fund of $10,500 for the establishment of a "University Press." The establishment of the University Press does not mean that the university will engage in the printing business, but merely means that funds of the press will be used to publish pamphlets and books on scientific and educational reports of the university, and that all such publications will bear the stamp, "The University of Wisconsin Press."

The grant also includes $8,000, which will provide for the continuation of the work now being done by Professor Aldo Leopold on game management and land-waste problems in connection with the university arboretum. Dr. H. L. Russell, director of the foundation, in a statement recently issued said:

> When it is realized that this organization started only ten years ago with no capital other than a single application in the U. S. Patent Office; that in this period of time it has built up a list of sixteen patents, not only in the United States and Canada, but in foreign countries as well; that it has developed a business organization with permanent offices in Madison, Chicago and New York, and has created an investment portfolio capable of yielding as interest over $160,000 this year, it is apparent that this method of handling university patentable ideas is being worked out in a unique way at the University of Wisconsin.

MEETING OF TEXAS GEOLOGISTS

THE Southwestern Geological Society, the Bureau of Economic Geology and the Department of Geology of the University of Texas were hosts to the geologists of Texas and adjacent states on February 13. A preliminary meeting was held at the Stephen F. Austin Hotel on Friday evening, at which the leaders of the three field trips briefly outlined the area to be covered. These were as follows: One to the central mineral region under the leadership of Dr. H. B. Stenzel; one to the Cretaceous in the vicinity of Austin under the direction of Professor F. L. Whitney, and one to the lower Tertiary to the east of Austin led by Dr. R. H. Cuyler. Approximately one hundred and forty geologists attended the various field trips.

Following the field trips, dinner was served at the University Commons. At the dinner Dr. H. Y. Benedict, president of the University of Texas, spoke briefly of his student days at the university under Dr. R. T. Hill, first professor of geology, and Dr. F. W. Simonds, Dr. Hill's successor. Both Dr. Hill and Dr. Simonds were guests at the dinner. At the close of Dr. Benedict's talk, portraits of both Dr. Hill and Dr. Simonds were presented to the department of geology to be hung in the seminar room of the geology building. Following the presentation of the portraits, Dr. Parker D. Trask, a graduate of the University of Texas, spoke on "Source Beds of Petroleum." Dr. Trask is an associate geologist with the United States Geological Survey and has been identified with the National Research Council in an investigation of the source beds of petroleum. Following his address the visiting geologists were conducted through the geology building and were entertained at an informal reception by the faculty and students.

FOSTER LECTURE FOUNDATION AT THE UNIVERSITY OF BUFFALO

MRS. ORRIN E. FOSTER, of Buffalo, has established and endowed as a memorial to her husband at the University of Buffalo a permanent lecture foundation in chemistry and allied sciences.

Long-time friends of the university, Orrin E. Foster and his family were the donors of Foster Hall, which was the first new building to be erected on the North Main Street Campus. The hall was dedicated on October 27, 1922, in connection with the inauguration of Chancellor Capen as head of the university. It has served as a laboratory for students of chemistry and pharmacy.

Since income from the new gift is now available, plans have been completed to inaugurate the foundation at once, with a series of public lectures, beginning in March. Four of the lecturers and their subjects are as follows:

March 18, Dr. Karl K. Darrow, of the Bell Telephone Laboratories of New York City, "Atoms and Elements."

March 23, Dr. Marston T. Bogert, professor of chemistry at Columbia University, "Around the World in Search of Perfumes."

April 6, Dr. Frank C. Whitmore, dean of physical sciences at Pennsylvania State College and president-elect of the American Chemical Society, "What Do the Organic Chemists Really Know?"

May 4, Dr. R. A. Gortner, chief of the Division of Agricultural Biochemistry at the University of Minnesota, "Recent Advances in Chemistry in Relation to Agriculture."

Speakers each year will be chosen by a Foster Lecture Committee composed of five members appointed by the chancellor. Members of the committee, who have just been appointed, are as follows: Dr. Groves H. Cartledge, head of the department of chemistry at the university, *chairman;* Dr. Harold G. Hewitt, assistant professor of chemistry; Dr. David F. Smith, professor of chemistry; Dr. Wilson D. Langley, associate professor of bio-chemistry; Dr. Preston Hoff, manager of chemical research at the du Pont-Rayon Research Laboratories in Buffalo.

THE INSTALLATION OF A CHAPTER OF SIGMA XI AT THE GEORGE WASHINGTON UNIVERSITY

AT the winter convocation of the George Washington University on February 22 a chapter of the Sigma Xi was installed. Dr. Edward Ellery and Dr. G. B. Pegram, who are national officers of Sigma Xi, conducted the exercises. Dr. Paul Bartsch, president of the petitioning group at the George Washington University, read the petition. Dr. Ellery presented the charter of the new chapter to its secretary, Dr. Paul Bowman, assistant professor of biology in the university. Dr. Pegram delivered the charge to the new chapter.

Forty-three members of the university faculty who were members of Sigma Xi chapters at universities where they took their degrees, and who formed the petitioning group, are charter members of the chapter. Fifty members have been elected. Delegates representing thirty-one chapters of the Society of the Sigma Xi in colleges and universities throughout the country were present at the convocation. Three hundred and fifteen students were graduated from the various colleges and schools of the university.

The convocation address was delivered by Dr. William Alanson White, professor of psychiatry at the George Washington University and superintendent of St. Elizabeth's Hospital. He spoke on "The Function of Education as Viewed from the Standpoint of the Present World Crisis."

At the ceremonies honorary degrees were conferred, President Marvin reading the following citations:

DOCTORATES OF SCIENCE

CHARLES GREELEY ABBOT: Persistent and courageous investigator in the field of astrophysics, in whom scientific thinking and religious reflections are not separated; secretary of the Smithsonian Institution.

PAUL BARTSCH: Eager scientist; teacher; quick to appreciate human values; who, for years, has devotedly served in the George Washington University.

WILLIAM BOWIE: Graduate of Trinity College (Connecticut); cheerful colleague and tried friend of those who seek for truth; to whom long experience in scientific endeavor has brought recognized leadership.

LYMAN JAMES BRIGGS: Sincere scholar and worthy public servant, in whom firmness of conviction and tolerance toward the beliefs of others go hand in hand; chosen to direct the Bureau of Standards.

GEORGE BRAXTON PEGRAM: Son of the South; well known investigator and expounder of physical sciences; worthy servant of Columbia University; acting-president of the Society of the Sigma Xi.

DOCTORATES OF LAWS

EDWARD ELLERY: Teacher, administrator and man of science, skillful in encouraging scientific endeavor, general secretary of the Society of Sigma Xi.

JOHN CAMPBELL MERRIAM: Master in the field of paleontology, painstaking leader in scientific investigation, philosopher and man of letters; devoted to the principles of natural law and fully cognizant of their application in the life of to-day; president of the Carnegie Institution of Washington.

WILLIAM ALANSON WHITE: Productive scholar, devoted teacher, enlightened executive, able to make knowledge a saving power in the lives of men; to know him is to desire his friendship.

SCIENTIFIC NOTES AND NEWS

DR. EJNAR HERTZPRUNG, director of the University Observatory at Leiden, has been awarded the Catharine Wolfe Bruce Gold Medal for 1937 of the Astronomical Society of the Pacific. He has also been appointed to the newly established Alexander F. Morrison memorial research associateship in the Lick Observatory.

DR. SELMAN A. WAKSMAN, professor of soil microbiology at Rutgers University, has been elected a corresponding member of the Academy of Sciences, Institute of France. He fills the place in the section of rural economy vacant by the death of Sir Arnold Theiler.

DR. A. N. WINCHELL, professor of mineralogy and petrology at the University of Wisconsin, and Dr. Douglas Johnson, professor of physiography at Columbia University, have been elected corresponding members of the Geological Society of China, Nanking.

DR. WILLIAM BERRYMAN SCOTT, professor of geology, emeritus, at Princeton University, celebrated his seventy-ninth birthday on February 12.

DR. HOWARD A. KELLY, professor of gynecology, emeritus, at the Johns Hopkins University, the only surviving member of the group that was instrumental

in the founding of the Medical School, celebrated his seventy-ninth birthday on February 20.

THE Western New York Section of the American Chemical Society has awarded the Jacob F. Schoellkopf Medal for 1937 to James G. Marshall, general superintendent of the Niagara and Welland plants of the Union Carbide Company and the Electro Metallurgical Company, in recognition of his "technical contributions to the calcium carbide and ferro-alloy industry, his untiring efforts in behalf of civic betterment and his unceasing interest in the improvement of working conditions." Formal presentation of the medal will be made early in October.

PROFESSOR J. R. SHANK, assistant director of the Engineering Experiment Station of the Ohio State University, received the Research Medal of the American Concrete Institute for his article entitled "The Plastic Flow of Concrete" at the annual dinner of the institute on February 26. This medal is awarded for the best research published in the proceedings of the institute during the year.

THE Alvarenga Prize for 1936 of the College of Physicians of Philadelphia has been awarded to Dr. Harry Eagle, passed assistant surgeon, U. S. Public Health Service, who is at present stationed at the Johns Hopkins Hospital. Dr. Eagle's Essay, "The Present Status of the Blood Coagulation Problem," will be published in a forthcoming issue of *Medicine*.

DR. CLARENCE J. WEST, technical editor at the Institute of Paper Chemistry affiliated with Lawrence College, Appleton, Wis., was presented with the gold medal of the Technical Association of the Pulp and Paper Industry at a luncheon given at the Waldorf-Astoria Hotel, New York City, on February 24. The presentation was made by Dr. Harry F. Lewis, dean of the institute and a member of the Technical Association. The medal was awarded to Dr. West for his work in the bibliography of technical and scientific literature dealing with pulp and paper.

DR. HENRY DWIGHT CHAPIN, professor emeritus of pediatrics at the New York Post-Graduate Medical School and Hospital, was the guest of honor at the annual dinner of the Faculty Association of the school on January 23. The occasion celebrated the fiftieth anniversary of Dr. Chapin's appointment as professor of pediatrics and his eightieth birthday, which was on February 4. The speakers at the dinner included Drs. Arthur F. Chace, president of the board of directors; Frederic E. Sondern, formerly president of the Medical Society of the State of New York; Adolph G. G. De Sanctis, president, and Walter T. Dannreuther, formerly president, of the Medical Society of the County of New York, and Charles Gordon Heyd, president of the American Medical Association. The Faculty Association presented to Dr. Chapin a silver loving cup.

DR. GEORGE W. MCCOY has been relieved as director of the National Institute of Health, effective on January 31. He had held the position since 1915, when the institute was known as the Hygienic Laboratory. It is reported that he will be assigned to investigations on leprosy.

DR. RAYMOND PEARL, professor of biology in the School of Hygiene and Public Health at the Johns Hopkins University, has been appointed Heath Clark lecturer for the year 1937 at the University of London.

DR. RONALD GEORGE WREYFORD NORRISH, Humphrey Owen Jones lecturer, has been appointed to succeed the late Professor T. M. Lowry as professor of physical chemistry at the University of Cambridge.

AT the University of London the title of professor of biochemistry has been conferred on Dr. A. C. Chibnall, in respect of the post held by him at the Imperial College, Royal College of Science, and the title of emeritus professor of mining geology has been conferred on Dr. C. G. Cullis, on his retirement from the university professorship of mining geology at the Imperial College, Royal College of Science.

DR. ADOLF BUTENANDT, professor of organic and biological chemistry at Danzig, has been made director of the Kaiser Wilhelm Institute for Biochemistry at Berlin-Dahlem.

EDWIN H. COLBERT, of the American Museum of Natural History, New York, has become associate curator of the department of geology and paleontology of the Academy of Natural Sciences of Philadelphia.

DR. JOHN C. HOSTETTER, who had charge of the construction of the 200-inch telescope mirror at the Corning Glass Works, has been made vice-president of the Hartford Empira Company, Connecticut.

DR. D. R. PYE has been appointed director of scientific research in the British Department of the Air and member for research and development in the air ministry, to take the place of H. E. Wimperis. W. S. Farren has been appointed deputy director of scientific research in succession to Dr. Pye.

DR. HARRY SOBOTKA, chemist to the Mount Sinai Hospital, New York City, and Dr. Louis J. Soffer have received a grant-in-aid from the Committee on Scientific Research of the American Medical Association for the study of lactic acid tolerance in hepatic disease.

THE Rockefeller Foundation has made a grant of

$3,000 to Professor Elizabeth Adams, of Mount Holyoke College, to be used during 1937 and 1938 for work on the endocrine glands.

Dr. A. A. Dunlap, assistant mycologist and plant physiologist at the Connecticut Agricultural Experiment Station at New Haven, has been granted leave of absence for four months for research to be carried out at the Johns Hopkins University on the sand culture of seedlings.

Dr. Paul Weiss has leave of absence from the University of Chicago from April 1 to October 1. He expects to continue his study of the nervous system in several European laboratories, principally at the laboratory of Professor Bremer in Brussels. The Rockefeller Foundation is sponsoring his work.

Dr. Sydney W. Britton, professor of physiology in the University of Virginia, has left for Panama for a two-months study of the wild animals of the Central American jungles. He is accompanied by the curator of the expedition, William Atkinson, and by Raymond F. Kline, research assistant. Two months will be spent at the Barro Colorado Laboratory, maintained on an island in Gatun Lake by Harvard University, the National Research Council and other organizations. While he is in the Canal Zone Dr. Britton has been invited to speak before the Society of Natural History of the Panama Canal.

The Institute for the Study of Malaria at Rome, of which Professor G. Bastianelli is director, has sent a mission to Abyssinia to study various aspects of malaria. Members of the mission include Professor G. Lega, pathology and clinical medicine, and Professor G. Raffaele, entomology, epidemiology and prophylaxis.

Dr. F. A. Vening Meinesz, president of the International Geodetic Association, lectured on "Gravity on the Atlantic Ocean" on February 16, before a joint meeting of the Society of Sigma Xi, District of Columbia Chapter, the Geological Society of Washington and the Washington Academy of Sciences.

Dr. Henry E. Sigerist, director of the Institute of the History of Medicine at the Johns Hopkins University, gave a Mayo Foundation lecture on "The History of the Concept of Disease" at Rochester, Minn., on February 26.

Dr. George H. Whipple, dean of the School of Medicine and Dentistry at the University of Rochester, will deliver the fourth annual Alexander Van Rensselaer lecture at the Drexel Institute of Technology on the evening of April 15. He will speak on "Blood Hemoglobin Production within the Body as Influenced by Diet and Other Factors under Experimental Conditions."

Dr. T. Wingate Todd, professor of anatomy at the School of Medicine of Western Reserve University, gave the first annual Clarence J. Grieve Foundation lecture on March 1. He spoke on "Facial Growth and Pharyngeal Health."

Dr. William T. M. Forbes, professor of entomology at Cornell University, gave on February 19 an address entitled "Butterfly Geography" before the Royal Canadian Institute, Toronto.

Dr. Richard P. Strong, professor of tropical medicine at the Harvard Medical School, delivered the first Chadwick Lecture of the Royal Society of Tropical Medicine and Hygiene, London, on January 21. His subject was "Onchocerciasis in Central America and Africa." Dr. Strong was recently elected an honorary fellow of the society.

Dr. Harlan T. Stetson, research associate at the Massachusetts Institute of Technology, gave on February 16 the first lecture in the Springfield, Mass., Public Forums Series entitled "Recent Developments in Science." The title of Dr. Stetson's lecture was "The New Universe."

J. G. Crowther, London, scientific correspondent of *The Manchester Guardian*, will give a series of public lectures on "The History of American Science," at Harvard University, during the first two weeks of March.

During the next five months Mills College, the second oldest college for women in America, will celebrate the eighty-fifth anniversary of its founding. The School of Science will be charge of the program for the month of March, while the various other schools of the college will plan the commemorative events for the other months. Among those who will take part are Dr. Robert Grant Aitken, emeritus director of Lick Observatory; Dr. Knight Dunlap, professor of psychology at the University of California at Los Angeles, and Dr. E. O. Lawrence, director of the Radiation Laboratory at the University of California.

The seventeenth International Congress of Agriculture will be held from June 16 to 23 at the Hague.

The fourth International Congress on the History of Science will be held in Prague from September 22 to 27, under the presidency of Professor Quido Vetter. Papers will be discussed dealing with the development of the sciences during the eighteenth and first half of the nineteenth centuries, the history of science in education and other historical subjects. The one hundred and fiftieth anniversary of the birth of Jan Evangelista Purkyně (or Purkinje), the distinguished Czech biologist, will be celebrated during the congress.

The fourteenth National Colloid Symposium will meet at the University of Minnesota on June 10, 11

and 12. Professor Herbert Freundlich, of University College, London, will be the guest speaker and will open the symposium. It is planned to hold the sessions in the Chemistry Building of the university on June 10 and 11, and then to drive to Rochester where the papers of the last day will be given at the Mayo Clinic. The sessions there will be devoted to papers of a bio-colloidal nature. Opportunity will be given to spend some time in visiting the Clinic Building and the research laboratories connected with the Mayo Foundation. The program for the symposium is rapidly being filled and it is suggested that authors, wishing to have papers considered for the meeting, should write at once to Professor H. B. Weiser, The Rice Institute, Houston, Texas. The local committee making arrangements for the symposium consists of D. R. Briggs, Geo. Glockler, R. A. Gortner, I. M. Kolthoff and L. H. Reyerson, chairman.

THE attention of scientific workers is called to the Theobald Smith award established by Eli Lilly and Company and approved by the council of the American Association for the Advancement of Science at the Minneapolis meeting. The conditions of the award as recommended by the executive committee after study of the situation and as adopted by the council are given in SCIENCE for July 26, 1935, p. 75. Members of the committee of award for 1936–37 are: Drs. Edwin G. Conklin, *chairman;* Anton J. Carlson, Howard T. Karsner, Chauncey D. Leake and Hans Zinsser. This committee will serve until it reports at the Denver meeting. By request of the committee and with the approval of the council all correspondence in the matter should be addressed to the Permanent Secretary and not to the individual members of the committee.

THE National Research Council announces that in the administration of its fellowships in the natural sciences, consideration will be extended to applications for fellowships of post-doctorate grade for the coming year in the fields of geology, paleontology and physical geography. The basic stipend will be $1,600 per year. Requests for application blanks should be addressed to the Secretary, National Research Fellowships Board in the Natural Sciences, National Research Council, 2101 Constitution Avenue, N.W., Washington, D. C. Applications to be considered should be returned to the Research Council by April 1.

AT the recent Atlantic City meeting both the Ecological Society of America and the American Society of Zoologists passed resolutions condemning the so-called "vermin" campaigns that are destroying some of our most interesting animals by the hundreds or thousands. The former society appointed one of its members to write certain articles for publication and wide distribution to call the attention of those interested in nature to what is being done.

SOIL survey reports and maps are in great demand and in many instances all available supplies are exhausted. Dr. Henry G. Knight, chief of the Bureau of Chemistry and Soils of the U. S. Department of Agriculture, urgently requests that any one having copies of these reports for which they have no further use send them to the bureau for distribution to people needing them. Upon request Government blanks will be forwarded for the purpose.

E. L. PACKARD, of Oregon State College, writes that the Oregon State Board of Higher Education has announced the establishment of the Institute of Marine Biology at Coos Head, on an eighty-five acre tract acquired for the purpose from the Federal Government through Congressional Acts of 1931 and 1935. Preliminary studies are said to indicate an abundant and diversified fauna and flora within the waters of Coos Bay and that the life of the open ocean shows a mingling of both northern and southern forms. The extensive marine fisheries include the salmon and the pilchard centering in this region, and the industries based upon crabs, clams and oysters afford many opportunities for basic researches in these fields. This station is organized to serve all the institutions of the State System of Higher Education of Oregon, and cooperatively others within the state and elsewhere, as a center of biological research relating primarily to the life of the sea and as a training ground for future biologists and teachers of biology.

DISCUSSION

DISSEMINATION OF SCIENTIFIC LITERATURE BY MEANS OF MICROFILMS

BECAUSE of the rapidly increasing volume of scientific literature, its classification and dissemination to those who use it is becoming an ever increasingly complex and costly problem. Aid of this character to research is usually rendered by means of abstract journals, but these require the cooperation of many persons, and their publication is very expensive.

It is believed that microfilms may serve both to supplement existing abstracts and, in cases where necessary, provide a more economical means of acquainting scientists with the work of others.

Although abstract journals enable the research worker to learn what papers have been published upon a given problem, they can give but few details in regard to each research. It is, therefore, usually necessary to consult the original paper, and this may be done only by those having access to well-stocked special libraries.

Another manner of acquainting workers with the literature of their subjects is by the publication of classified catalogues of the titles of scientific papers appearing in the journals of the world. Although the title alone certainly gives less information than can be included in an abstract, it is a question whether such additional information is really worth its far greater cost. Those concerned with furnishing such service to research workers are faced with the question as to whether, at an approximately equal expenditure of effort, it is preferable to provide a completely classified catalogue of the titles of scientific papers in a given field or abstracts of only those papers selected as worthy of the attention of research workers. A decision of this question will be influenced by the availability of the original papers to those who need them. Certainly no one who can consult the original paper would be content with the abstract; consequently it may be expected that a service which provides a highly classified list of scientific papers and also furnishes at very low cost photographic copies of the original papers themselves will be preferred by many to even the most perfectly organized abstracting service.

Thanks to the perfection in recent years of apparatus for the production and reading of microfilms, it is suggested that such equipment may be adapted to providing the kind of complete service to research workers mentioned above.

The organization of a service of this kind, based on microfilms, falls into two divisions: that of providing a classified and indexed catalogue of titles of papers and that of furnishing microfilm copies of the papers themselves. The establishment of such a service would also result in the gradual accumulation of complete microfilm collections (filmothecs) of scientific literature. In this connection, the question arises as to whether it is preferable to develop services and build up collections in the separate branches of science, or undertake the organization of a single all-embracing service and filmothec of science. Due to the immensity of the latter plan, it seems preferable to confine attention at this time to filmothec service in single branches of science.

Even in the most restricted field of research, and certainly in the larger ones, it would first be necessary to establish subclasses of the subject. These would be given abbreviated distinctive descriptions as well as numerical (decimal) designations. A widely published key to these descriptions and corresponding numerical designations might eventually make it possible to rely exclusively upon the numerical classification just as is done at present in the arrangement of books upon the shelves of libraries in accordance with the Library of Congress classification.

In the operation of a "Filmothec Service," there would be required only such trained scientists as would be needed to assign correct classifying designations, and choose the best indexing words for the original papers published in the current scientific periodicals. When this was done and the corresponding numerical designations and indexing words stamped on or affixed to each article, all other steps in the process could be executed by persons without special technical knowledge.

The current journals, after having their constituent original articles provided with the classifying designations, would be delivered to the photographic laboratory and each article successively photographed with its identification numbers and indexing words. The finished band bearing these microfilm copies would then be cut in lengths corresponding to each separate article. These individual microfilm strips would be filed in appropriate folders, each bearing the designation of the subclass and having serially arranged pockets to receive the serially numbered individual strips. By this means the articles in a particular branch of science would automatically be collected together and serially numbered. When a given folder was filled or at such periods of time as chosen, the titles and designation numbers of the articles in each folder would be typewritten in their regular numerical order, and an index of the selected words of each title prepared. These typewritten copies of the contents of each folder would be assembled and prepared for distribution to research workers in whatever form was most economical. For a very large and active branch of science, these lists would undoubtedly be printed and distributed in editions of many thousands. For more restricted fields of research, the lists might be reproduced by offset printing or even by duplicating machines.

By means of these highly classified lists of titles of articles which would be indexed in accordance with the particular subject treated in each article, the individual worker would be able to choose those articles which he considered of most interest to him. To obtain copies of these, he would only have to send the designating numbers of the articles he desired to the office in which the master microfilm copies were on deposit. Upon receipt of such orders, the desired original microfilm strip would be removed from the folder and sent to the photographing laboratory for making positive film copies of the original, or enlarged photoprint copies, if preferred. This process is extremely simple

and can be carried out at considerably less cost than making the photographic copy direct from the journal. The positive copies thus made would be sent to those requesting them, and the master negative returned to its proper pocket in the folder in which it is kept.

In the above brief outline of the general organization of a Filmothec Service, it has not been possible to go into detail in regard to the several mechanical operations. The experience so far gained, however, with microfilm cameras shows that no serious difficulties may be expected. It is simply a question of the systematic organization of the work and the application of ordinary ingenuity in perfecting systems of identification markings for the film strips and properly constructed folders for filing and preserving the master negatives.

The technical directions necessary and the cost of making the original microfilms by directly photographing the periodicals should not greatly exceed the cost of preparing the card catalogues of original papers which would be necessary as the basis for the preparation of catalogues of scientific literature. The cost of making positive copies from negative microfilm strips is certainly far less than making microfilm copies directly from the journals.

This plan also has the advantage that complete filmothecs of scientific literature would be gradually built up, and with succeeding years, there would be a greatly increased amount of literature of which positive copies of desired papers could be supplied at an exceedingly low cost.

The one objection which may be offered is that no one library receives all the journals which must be consulted in the preparation of complete catalogues of titles of papers in a given field of science. Although this is true, there has developed such a spirit of cooperation between American libraries that by choosing two or three of the most important, and setting up microfilm cameras in these, the few journals which would still be necessary could probably be secured by loan or by purchase. Furthermore, this is a plan which lends itself especially well to making collections and disseminating literature for the benefit of research workers in very restricted branches of science. There are, for example, certain very special journals which, in addition to publishing original articles in their fields, also furnish abstracts of papers published elsewhere. This is, of course, a distinct service, but certainly of considerably less value than would be the providing to their subscribers of microfilm copies of the original articles chosen for abstracting.

It is evident that the suggested plan of using microfilms for the dissemination of scientific literature is capable of first being tested on a very limited scale, and if its value is demonstrated by such experience, may be expanded as rapidly as the results justify. It offers possibilities which are worthy of serious consideration by those concerned with the more perfect utilization of scientific literature.

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MICRO-PHOTOGRAPHY OR PHOTO-MICROGRAPHY?

RECENT issues of SCIENCE have brought suggestions on scientific nomenclature and the use of English. In this connection I would suggest for relegation to oblivion that horrible hybrid "photo-micrography." I have never been able to see the reason for coining this cacophonous misnomer—surely the term micro-photography is descriptive enough. One shudders to think what would happen if the precedent set by photo-micrography were followed consistently: we should then have to drop color photography and celestial photography in favor of photo-chromography and photo-uranography. If we sanction photo-micrography astronomers will be at a loss to know whether chronograph refers to an instrument used in measuring time or to a picture of Saturn, and we may, perhaps, look forward to the day when the studio photographer who specializes in portraits will announce himself as an expert in photo-prosopography.

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A VASOPRESSOR LOCAL ANESTHETIC

DR. RAYMOND L. OSBORNE and his associates are to be congratulated for the successful synthesis of a vasopressor local anesthetic, as described in SCIENCE, (85: 105, January 22, 1937). A brief historical account is included in the report. The reasons are given for attempting the synthesis of a chemical agent which combines the local anesthetic actions of the alkamine esters of para-amino benzoic acid with the vasoconstrictor effects of the phenylethylamines. Since the report does not refer to any other attempts to synthesize such a compound, it gives the impression to the uninformed that it is the first report on this matter to appear in scientific literature. This is unfortunate. Other efforts, more or less successful, of this same sort have been made by Kubota,[1] Takeda,[2] Hartung, Munch and Kester,[3] and in a particularly exhaustive manner by Alles and Knoefel.[4] The latter discuss 29 compounds of this type which were deliberately prepared

[1] Kubota, *Jour. Pharm. and Exp. Ther.*, 12: 361, 1919.
[2] Takeda, *Jour. Pharm. Soc. Japan*, No. 426, 691, 1917, through *Chem. Abs.*, 11: 3241, 1917.
[3] Hartung, Munch and Kester, *Jour. Amer. Chem. Soc.*, 54: 1526, 1932.
[4] Alles and Knoefel, *Arch. Internat. Pharmacody. Ther.*, 47: 96, 1934.

to accomplish the purposes described by Osborne. These drugs were studied in a thorough way in order to determine which of the group would be most satisfactory for clinical trial. One of these vasopressor local anesthetics of Alles and Knoefel, β-benzoyloxy-β-phenyl-ethyl-demethylamine hydrochloride, came to clinical use and was reported upon by E. W. Ferber.[5] The purpose of this present note is not to detract from the merit of the report of Dr. Osborne and his associates. It does seem, however, that his statement in SCIENCE would have given a more correct impression had it referred to previous work of the same character.

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THE ACTION OF ESERINE AND ITS ANALOGUES ON SKELETON MUSCLE

IN SCIENCE for December 18, 1936, p. 551, Morison and Rosenblueth deal with the cause of their earlier failure (Rosenblueth, Lindsley and Morison)[1] to detect the potentiating effect of eserine and its analogues on the response of a mammalian muscle to a motor nerve volley. Brown, Dale and Feldberg[2] had suggested that the anesthetic might have been responsible for the absence of this action in Rosenblueth, Lindsley and Morison's experiments; but Morison and Rosenblueth have now found that the length of the interval between successive motor nerve volleys is a much more important factor. This observation we had made ourselves even before the paper by Brown, Dale and Feldberg was published, and we have dealt fully with the point in a paper which has for some months been awaiting publication in the *Journal of Physiology*. A preliminary account of the observation had, indeed, already been given by one of us (Z. M. B.) in a review published as long ago as October 11, 1936, in *Liége Médical*.[3] The journal in question has probably not a wide currency, and we welcome the note by Morison and Rosenblueth, as showing that they had independently confirmed our observation.

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SCIENTIFIC BOOKS

RECENT PHYSICS

The Renaissance of Physics. By KARL K. DARROW, pp. 306, $3.00. The Macmillan Company, 1936.

THE author of this volume is no stranger to the world of physics. Eight or ten years ago a stream of luminous essays poured forth from the Bell Telephone Laboratories under the title of "Some Contemporary Advances in Physics." These immensely helpful papers, directed mainly to his professional colleagues, disclosed not only a remarkable grasp of the various fields of recent physics but also a rare mastery of the art of exposition.

The present volume, however, is the outgrowth of a series of Lowell Lectures, greatly amplified but still addressed to the intelligent reader whether with or without laboratory experience. It is indeed one of those rare messages which contain much for the beginner and much for the expert, reminding one, in this respect, of Maxwell's "Matter and Motion" and Tait's "Recent Advances in Physical Science."

If the merits of a volume are to be appraised upon the matter which is chosen for discussion, upon the worthiness of the treatment and upon the tenacity with which it holds the interest of the reader, Dr. Darrow's "Renaissance of Physics" must take high rank. For the task undertaken is the explanation, in words of one syllable, of the rapid evolution of physics during the last fifty years; but this is to be done by building upon foundations already firmly established; and there is to be no discontinuity with the classical physics. "The continuity of thought," he says, "the partial adequacy of old ideas to new discoveries—these have outrun anything which the physicists of the past could reasonably have foreseen." The entire discussion is based upon the solid ground of mechanics, heat and waves. The spirit of the author is well exemplified at the very outset by the manner in which he wisely detours the definition of physics, with all its metaphysical quagmires, and, in its stead, defines a physicist as "some one who uses his senses for observing; mechanical and thermal instruments for measuring; and mathematics, especially the mathematics developed in the service of physics, for reasoning. I say nothing about a limitation of the subjects of his inquiry; there is none—he is authorized to use his methods and his mathematics on anything whatsoever."

The second and third chapters lead up through Gilbert Faraday, Hittorf, Crookes and Edison to J. J. Thomson and "the release of electrons from matter . . . an event . . . of transcendent importance." Here again I quote a thoroughly pragmatic definition to illustrate the beautiful accuracy with which the author fits his language to his purpose. "May we say," he

[5] Ferber, *Jour. Amer. Dental Assoc.*, 23: 788, 1936.
[1] A. Rosenblueth, D. B. Lindsley and R. S. Morison, *Amer. Jour. Physiol.*, 115: 53, 1936.
[2] G. L. Brown, H. H. Dale and W. Feldberg, *Jour. Physiol.*, 87: 394, 1936.
[3] Z. M. Bacq, *Liége méd.*, No. 41, p. 1173, Oct. 11, 1936.

asks, "that 'vacuum' shall stand for any density of gas, however high or low, provided only that what is left of the gas in the tube is not interfering at all with the phenomena which we happen to be observing?" The discovery of the Edison effect is alluded to as "one of the observations which change the course of history."

A fine illustration of the manner in which a discussion can be made quantitative without the use of mathematical symbols is found in chapter IV, where various matters beside electrolytic conduction and the size of the electronic charge are taken up. One factor which contributes largely to the outstanding clarity of the book is the use of excellent metaphors and similes. Here, for example, is the ferryboat upon which electrons travel; and again a metal described as "a forest in which the motionless trees stand for atoms with positive charges, while the footloose electrons correspond to roving wild beasts." No man, if he reads this while he is awake, can lay the book down without having a clearer conception of what happens in the photoelectric effect and in thermionic emission from incandescent metals.

"The Mystery of Waves and Corpuscles" is the heading of Chapter VIII; and the mystery largely remains —I think the author himself will agree with me—when the end of the chapter is reached. Here, if anywhere, it would appear that some discontinuity between classical and recent physics must be admitted. When the two fundamental equations of Einstein

$$E = h\nu \quad \text{and} \quad p = h/\lambda$$

come up, the author frankly confesses that the problem of "making sense" of these two rules is "a very tough one." "So tough is it," he says, "that physicists have been driven to all manner of singular devices. It is the origin of most, if not all, of the amazing and baffling assertions which have crept into popular literature—sources of grief to those who expect a classic sobriety of statement from the scientist, and of malicious joy to those who like to see unsettlement and incoherence invading an authoritative science."

The paragraph just quoted, coming at the end of a brilliant chapter, easily turns one's mind to the following remark of Professor Max Born in his "Atomic Physics" (p. 120) that "There is naturally no way of deducing the wave-equation by strict logic; the formal steps which lead to it are merely matters of clever guessing."

The structure of the atom is taken up in Chapter IX, which opens with the following thumbnail sketch of this complex microcosm:

> An atom is a constellation of negative electrons organized and held together by the attraction of a positive charge ensconced upon a nucleus which is the center of the constellation and is many times more massive than all the electrons put together.

Here before the end of the chapter is reached Dr. Darrow lays down the solid foundations of spectroscopy and clearly enunciates the basic principles which underlie the work of Aston and Dempster. He also embraces this opportunity to pay his respects to Pythagoras by imagining the delight with which this ancient Greek would have listened to the story of Moseley's discovery that the various elements differ one from another, mainly in a number which is attached to each by definite experimental fact.

The next two chapters are devoted to "the masterpiece of the contemporary group of physicists," *the transmutation of the elements*. Here, in following the work which Lord Rutherford initiated in 1919, the author is at his best and even outdoes his own previous achievements in clear and picturesque exposition. Witness the following account of the production of radio-sodium in the cyclotron of Lawrence and of the evidence for thinking that a neutron lurks in every deuteron:

$$_1H^2 + {}_{11}Na^{23} \longrightarrow {}_{11}Na^{24} + {}_1H^1$$

upon which he comments, "One may imagine that the neutron is charioted by its attendant proton to the portals of the nucleus through which it slips while its companion is barred out." This penultimate chapter carries the happy title, "Victory over the Elements"; and the volume is fitly brought to a close by one on "The Unity of Nature," in which the remarkable theorem of Einstein, that "Any energy E has a mass E/c^2; any mass m has an energy mc^2," is established in beautifully logical sequence and in carefully chosen words. The reader is here introduced to the crowning achievement of twentieth century physics—the establishment of each of the great laws of conservation of matter and of energy by uniting them into one single valid general principle.

So much for the contents and style of this admirable book, characterized by accurate scholarship, perfect clarity, fine perspective and fascinating diction. If I were an instructor in English composition, I would probably disapprove of the efflorescence of parentheses which meets the eye in every chapter; this on the principle that the introduction of round brackets is, in general, a confession of obscurity in the structure of the sentence. But those of us who have read or listened to Dr. Darrow know how deftly these asides are woven in, how much they add to clarity and how cleverly they protect the reader from wrong impressions.

When one is just arising from a bountifully spread table, nothing could be more ungracious than to complain because some additional course had not been served. So I am not going to ask for an additional

chapter, but merely remark that these eleven chapters whet one's curiosity as to what comment the author would make upon the view of Sir J. J. Thomson ("Recollections and Reflections," pp. 368–9) that "the mass, momentum and energy of a charged sphere are distributed throughout the medium around it and not in the sphere itself."

When the present administration has completed its investigation of the methods of the American Telephone and Telegraph Company it is to be hoped that they will find both reason and space for commending the policy of any corporation which has the wisdom, foresight and ability to maintain a research staff of the caliber of the men now at the Bell Telephone Laboratories and of their late director, to whom the book is so appropriately dedicated.

HENRY CREW

NUCLEAR PHYSICS

An Introduction to Nuclear Physics. By N. FEATHER. x+213 pp. 21 figs. 3 plates. New York: The Macmillan Company; Cambridge: The University Press. 1936. $3.00.

RADIOACTIVITY was discovered in 1896 by Becquerel in connection with studies concerning the nature of fluorescence. Later developments, coming as a result of this discovery, have been of benefit both to the medical profession and to physicists and chemists wishing to gain an insight into the fundamental properties of matter. Alpha-particles, obtained from radioactive elements, were the tools with which Lord Rutherford was able to perform his scattering experiments, the results of which led to the famous nuclear atom (1911). This hypothesis was the foundation stone upon which Bohr was able to build his theory of the hydrogen atom. The tremendous developments brought about in physics, chemistry and astronomy as a consequence of Bohr's theory are now known to every one.

While rapid strides were being made in understanding the outer structure of the atom, steady progress was also being made in our knowledge of the nucleus, albeit this work was somewhat eclipsed by the great volume of papers in the other field. Since the discovery of the positron, the neutron, artificial radioactivity and transmutations by high velocity particles, the amount of work being published on nuclear physics is beginning to rival that produced during the atomic structure "boom" of the 1920's. The author of the present volume has made very notable contributions to the study of nuclear physics.

With experimentation in nuclear physics going forward at such a terrific rate, one would have a certain feeling of futility in attempting to write a book on the subject, since it would be supposed that the book would be out of date before it could possibly be published. It takes rare skill on the part of the author to choose material which will be fundamental and at the same time up to date, and a considerable amount of speed on the part of the publisher to get the book off the press and distributed in a very short length of time. This rare feat has been accomplished in a singularly successful manner by Dr. Feather and the Cambridge University Press.

The book is divided into four parts, the first of which is introductory in character, giving the necessary background for an understanding of the subject. The next three parts treat in turn stable nuclei, unstable nuclei and transformations produced by fast-moving particles and by radiation.

The superbly written introductory chapters give an excellent description of the development of atomic and nuclear physics during the last forty years. At the outset the author describes in detail the types of measurements made in experiments on nuclear physics, showing clearly the relation of the quantities measured to our well-known standards of length and mass. In these introductory chapters all branches of physics having to do with the study of the nucleus are concisely discussed, the essential points being introduced in such a way that the fundamentals are clearly before the reader at all times. After a discussion of the important experiments on radioactivity and scattering of alpha-particles, the results are considered in the light of the classical theory—including also the Bohr theory. The shortcomings of the classical theory in accounting for certain experiments in nuclear physics are pointed out and the wave mechanics is then introduced from an experimental point of view, without recourse to mathematics. The general results of the theory are given and the uranium paradox, resonance capture, and the scattering of identical particles are treated. The closing chapter of Part I, entitled "Elementary Particles: Nuclear Structure," contains an account of the various suppositions made as to the constitution of nuclei, the laws of force between elementary particles, and a particularly enlightening discussion of the nature of the Heisenberg-Majorana exchange force.

Part II deals with the measurement of nuclear masses by means of the mass spectrograph and by optical methods, together with a discussion of the determination of masses by means of the energy balance in disintegration experiments. The chapter on nuclear spins and moments is somewhat inadequate, but gives the main results obtained in this field, together with a table of nuclear spins and moments.

Part III deals with the emission of alpha-particles, of electrons, positive and negative, and of gamma rays. Since other treatises exist in which the subject of the emission of alpha-particles and gamma rays is

treated in great detail, the present section merely states the main experimental facts and brings the material up to date. The chapter on the emission of positive and negative electrons completes the usual discussion of beta-ray spectra by the inclusion of data on the emission of positrons and electrons from light elements made radioactive by bombardment. The foundations and results of Fermi's theory of beta particle disintegration are clearly discussed and the connection between this theory and Sargent's curves is pointed out. The recent experiments concerning the existence (or perhaps non-existence) of the neutrino are described.

The final section of the book—Part IV—deals with transformations produced by high energy particles and light quanta. The effects produced by alpha-particles, neutrons, accelerated particles and quanta are described in turn. The discussion is well ordered and entirely free from the confusion with which one is confronted if one has to go to the original source for information. The usefulness of these sections is enhanced considerably by the inclusion of a section entitled "collected results" at the end of each chapter. These sections contain in tabular form all the useful information which has been accumulated to date on induced disintegration.

The book contains an unusually complete list of references. In fact, references to papers published in this country as late as July, 1936, and to English papers published after the book was printed are to be found. The theoretical discussions are treated mostly from the physical standpoint without the use of mathematics. The book should be of great interest both to the specialist in nuclear physics and to one who merely wishes to find out what the nuclear physicist is doing.

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SPECIAL ARTICLES

VITAMIN B_1 AND THE GROWTH OF EXCISED TOMATO ROOTS

IN 1922 one of the authors[1] reported a method of cultivating excised root tips under sterile conditions, and found the growth of excised roots of corn (*Zea mays* L.) to be limited in extent in a modified Pfeffer's solution containing dextrose. As a working hypothesis it was assumed that oxygen, the mineral salts of Pfeffer's solution, glucose and water were insufficient for the continued growth of excised corn roots. In the same year Robbins[2] reported the beneficial effects of autolyzed yeast on the growth under sterile conditions of the excised root tips of corn. Various evidences were presented and discussed, indicating that the yeast was effective by furnishing some accessory growth factor or factors originally supplied to the root tip from the grain but fractionated in subculturing the root tips. The same hypothesis was considered in later papers. However, it was not possible at that time definitely to determine in what the effectiveness of the yeast consisted.

White[3] demonstrated potentially unlimited growth for excised root tips of tomato (*Lycopersicon esculentum* L.) in a solution containing mineral salts, cane sugar and yeast.

Since September, 1935, we have cultivated excised tomato root tips in White's solution, thus confirming his original report of the possibility of unlimited growth of the root tips of this plant. We have found that excised tomato root tips which have been subcultured for some time will not grow in White's solution if the mineral salts are omitted, leaving the cane sugar and yeast only. They will not grow if the cane sugar is omitted; nor will they grow if the yeast is omitted and only cane sugar and mineral salts are present.

The effective materials in the dried yeast are not soluble in absolute ethyl alcohol but are soluble in 80 per cent. alcohol. The residue left after extraction with absolute and 80 per cent. ethyl alcohol is ineffective. The beneficial action of the yeast is not eliminated by autoclaving for 12 hours at 120° C. at pH 9.0. Yeast ash prepared by ashing at low red heat in a muffle furnace will not replace the yeast.[4] However, we have found excised tomato roots to grow in White's solution in which the yeast is replaced by natural crystalline vitamin B_1 Merck or by synthetic vitamin B_1 Merck.[5] The vitamin is effective at great dilution. Growth of root fragments has been secured in 50 cc of White's solution without yeast to which $1 \times 10^{-4}\gamma$ of natural crystalline vitamin B_1 Merck was added. No growth was secured in the same solution lacking both yeast and the vitamin. The vitamin in this solution was present at a concentration of 1 part in 5×10^{11} parts of liquid or $2 \times 10^{-6}\gamma$ per cc, a dilution of the same order as that found by Kögl[6] to be effective for

[1] William J. Robbins, *Bot. Gaz.*, 73: 367–390, 1922.

[2] *Idem., Bot. Gaz.*, 74: 59–79, 1922.

[3] P. R. White, *Plant Physiol.*, 9: 585–599, 1934.

[4] The authors are indebted to F. Kavanaugh for the preparation of the yeast fractions and for other assistance.

[5] The authors express their appreciation of the assistance of R. R. Williams in securing this material and to Merck and Company for the gift of the synthetic vitamin B_1.

[6] Fritz Kögl, and Benno Tönnis, Hoppe-Seyler's *Ztschr. f. physiolog. Chem.*, 242: 43–73, 1936.

crystalline biotin on yeast. We have not yet determined the lower limit of effectiveness, though we have found concentrations up to 1γ per cc to be harmless and little better than the more dilute solutions. The extreme dilution at which the vitamin is effective accounts for our failure to inactivate yeast by long heating under pressure at an alkaline reaction; the vitamin is not completely destroyed under such conditions and sufficient remains to be effective. The insolubility in absolute alcohol of the effective factors in the dried yeast is probably apparent only. That the vitamin and not some contaminant is the effective agent would seem to follow from the dilutions at which the vitamin is used and because the contaminants are probably not the same in the synthetic and natural products, both of which are effective.

The beneficial effects of yeast on the growth of the root tips is not completely accounted for by the vitamin B_1 content of the yeast, since the growth in White's solution containing yeast is better than in the same solution in which the yeast is replaced by vitamin B_1. Part of the beneficial effect of the yeast is due to the ash elements in it, since growth is improved by the addition of yeast ash to White's solution in which the yeast is replaced by vitamin B_1 and the growth in the latter solution is improved also by the addition of supplementary mineral elements. We have not yet determined whether amino acids, hormones or vitamins other than B_1 present in yeast play a significant rôle in the cultivation of excised tomato root tips.

Our results show that excised tomato roots grow if supplied with water, oxygen, mineral salts, carbohydrate and vitamin B_1. These results are similar to those of Schopfer[7] for *Phycomyces Blakesleeanus* which he has found requires vitamin B_1, though this organism apparently requires asparagine also in addition to mineral salts, carbohydrate and the vitamin. It appears that a medium containing mineral salts, carbohydrate and vitamin B_1 is adequate for the unlimited growth of tomato roots. We have grown excised tomato roots successfully for five months in such a medium through four subcultures without evident diminution in growth rate. Such a medium is composed of known constituents and may be regarded as a synthetic medium adequate for the growth of tomato roots.

Whether any substitute for vitamin B_1 can be found remains to be determined. Present evidence indicates that pantothenic acid (a highly purified sample furnished the writers by R. J. Williams) can not be substituted for vitamin B_1. Another aspect of the problem of general interest and significance is the possible occurrence of vitamin B_1 in highly purified sugars. Judging from the response of excised tomato roots, growth factors (probably vitamin B_1) are present in samples of purified maltose and dextrose secured from various companies. Hall, James and Stuart[8] have reported the occurrence of growth stimulants for yeast in white sugar, and Schopfer[7] found a growth factor for *Phycomyces* in maltose.

Our results confirm the working hypotheses originally formulated to explain the beneficial action of yeast on the growth of excised roots in a mineral solution containing sugar and demonstrate that the parasitic relationship of the tomato root to the top involves both carbohydrate and vitamin B_1.

Whether the medium the writers have used is adequate for all types of plant tissue remains to be seen; the results, however, are significant in presenting a synthetic solution apparently adequate for the growth of one type of plant tissue and should be of interest to those engaged in the cultivation of isolated plant embryos and other portions of higher plant tissues.

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THE SIGNIFICANCE OF THE ADRENALS FOR ADAPTATION[1]

IN the course of our studies on the alarm reaction[2,3] we found that the symptoms of adrenal deficiency are almost identical with those observed in non-adrenalectomized animals after exposure to serious damage. Decreased blood pressure; accumulation of water in tissues, with a simultaneous loss of water from the blood; decreased blood sugar; decreased body temperature; muscular weakness; formation of gastric and intestinal ulcers; are the most characteristic features both of adrenalectomy and of the alarm reaction. We have to conclude, therefore, that there is no specific change characteristic of adrenal deficiency, and that the changes observed after the removal of the adrenals are simply due to the response of the organism to general damage—that is to say, they are changes caused by the alarm reaction. We found, furthermore, that alarming stimuli (*i.e.*, stimuli which would elicit an alarm reaction) will cause much more pronounced changes in adrenalectomized animals than these same stimuli would be able to produce in the normal. This

[7] W. H. Schopfer, *Ber. deut. bot. Ges.*, 52: 308–313, 1934.

[8] H. H. Hall, L. H. James and L. S. Stuart, *Jour. Ind. and Eng. Chem.*, 25: 1052–1054, 1933.

[1] A detailed publication of the experimental data mentioned in this letter will shortly appear in the "Archives Internationales de Pharmacodynamie et de Thérapie."

[2] H. Selye, *Can. Med. Assoc., Jour.*, 34: 706, 1936; *Nature*, 138: 32, 1936; *Brit. Jour. Exper. Pathol.*, 17: 234, 1936; *Endocrinol.*, in press.

[3] H. Selye, C. Harlow and J. B. Collip, *Endokrinol.*, 1936.

in itself suggested that the function of the adrenals is to increase the resistance of the organism to alarming stimuli; furthermore, we observed that a marked enlargement of the adrenals is a constant result of exposure to damaging stimuli, an observation which corroborates the assumption that these glands play an important rôle in the defence of the organism against alarming agents.

It has been claimed that the adrenal is particularly important for the maintenance of normal body temperature, for the prevention of fatigue following exhausting muscular exercise, for the detoxification of various harmful substances and for the maintenance of a constant blood sugar level. Exposure to variable surrounding temperature, excessive muscular exercise, toxic doses of various drugs or agents apt to cause hypoglycemia, act as alarming stimuli. Consequently a decreased resistance to such agents is to be expected if we assume that the function of the adrenals is to prevent the damaging effects of the alarm reaction. But this evidence was not sufficiently conclusive to justify such a theory. We therefore proceeded to experiment with a large number of rats which we adapted to cold, muscular exercise or various drugs. Since damaging agents cease to elicit an alarm reaction after adaptation has occurred, we could expect this experiment to show whether the damage caused by a certain stimulus in an adrenalectomized animal is the result of the stimulus as such or of the alarm reaction which it produces. We found that animals previously adapted to such stimuli as muscular exercise, cold or toxic doses of drugs will tolerate exposure to these same stimuli very well, even after the adrenals have been removed, while exposure to the same stimuli invariably kills not-adapted, adrenalectomized controls, with symptoms characteristic of adrenal insufficiency and of the alarm reaction.

We concluded from these observations that one of the most important functions of the adrenals is to increase the resistance to alarming stimuli. Since most stimuli are "alarming" when the organism is first confronted with them, the adrenals play a very important rôle in the first stage of adaptation to the conditions of the environment. After this first stage, however, the changes necessary for the acquisition of further adaptation take place in the peripheral tissues. In this stage, the stimulus ceases to be "alarming" and therefore the adrenal hormones are no longer required for the process of adaptation.

We assumed that the symptoms of the alarm reaction are mainly due to the liberation from the tissues of some toxic metabolite (possibly histamine or some physiologically similar compound). If this assumption should prove to be correct, one would have to conclude that the function of the adrenals is to detoxify this metabolite. In this event, there would be no contradiction between our interpretation of adrenal deficiency and the intoxication theory as such or possibly even its more modern modification, the histamine intoxication theory (Lucas). The circulatory theory (Swingle and co-workers) and the carbohydrate theory (Britton and Silvette) consider one symptom of the alarm reaction—the circulatory disturbance in the first case and the hypoglycemia in the second case—to be the basic cause of all the deficiency symptoms. In the light of our findings, these seem to be the result rather than the cause of the deprivation syndrome. The cause of it is the decreased resistance to alarming stimuli. That is why all these deficiency symptoms will appear in adrenalectomized animals at a time when they would otherwise not be evident—that is, immediately following exposure to an alarming stimulus. It seems quite likely that the loss of sodium which is the basic change according to those who believe in the sodium deficiency theory (Loeb, *et al.*) or the increase in potassium (Zwemer) or the deficiency in phosphorylation (Veszar) or the increase in non-protein nitrogen—all of which have been considered to be the primary change—are also symptoms rather than the cause of adrenal insufficiency.

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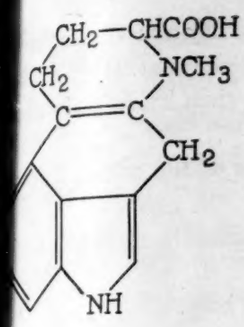
THE SYNTHESIS OF SUBSTANCES RELATED TO LYSERGIC ACID

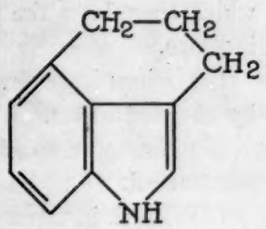
A VERY probable structure for lysergic acid (formula I), the characteristic constituent of the ergot alkaloids, has been derived from the interpretation of, among other things, its characteristic groupings and degradation products.[1] Recently attempts have been made to substantiate this formulation by synthesis. As a first step we have checked the possibility of the production of that portion of this structure composed of the 3-ring system, 3,4-trimethylene indole (formula II). A method to accomplish this was found in the reduction of naphthostyril (the lactam of 8-amino-1-naphthoic acid) with sodium in butyl alcohol. As a by-product a substance normally to be expected was simultaneously formed, *viz.*, 1-hydroxymethyl-8-amino-1,2,3,4-tetrahydronaphthalene. The identities of these substances were shown by their production by a different procedure, namely, reduction of the methyl ester of 8-amino-1,2,3,4-tetrahydro-1-naphthoic acid.

Trimethylene indole exhibits the usual indole reactions but not, however, the characteristic Keller reaction given by the ergot alkaloids (lysergic acid). A nearer approach to the synthesis of lysergic acid

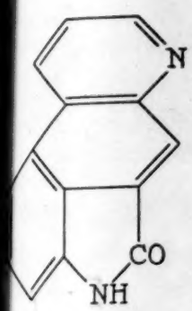
[1] W. A. Jacobs and L. C. Craig, *Jour. Biol. Chem.*, 115: 227, 1936.



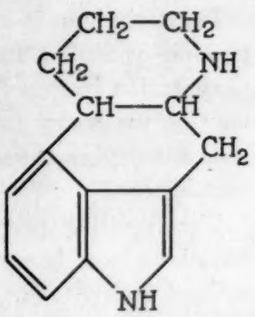
I



II



III



IV

itself has more recently been achieved as follows. 3-amino-1-naphthoic acid[2] by the Skraup reaction has given the corresponding β-naphthoquinoline carbonic acid which on nitration yielded a nitro-β-naphthoquinoline carbonic acid. The position occupied by the nitro group became evident after its reduction to the amino group, since lactamization then readily occurred with production of the substance given in formula III. In preliminary experiments, reduction of the latter with sodium and butyl alcohol yielded a mixture containing a substance apparently with the structure given in formula IV, since this mixture gave color reactions closely approaching those which are so characteristic of lysergic acid and its derivatives.

There is now in progress in this laboratory a logical extension of this work to include other substances related to lysergic acid and eventually to lysergic acid itself.

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SCIENTIFIC APPARATUS AND LABORATORY METHODS

AN AUTOMATIC DEHYDRATING DEVICE

A SIMPLE and satisfactory apparatus whereby histological and cytological tissues may be dehydrated gradually has been devised, thus preventing any shrinkage which may be due to improper upgrading in the alcohol series. By this method tissues for paraffin impregnation may be run up in twelve hours to three days, the length of time depending upon the rate of flow from the siphon.

Apparatus used: A 500 cc flask (A) filled with absolute alcohol is fitted with a one-holed rubber stopper to which is connected a piece of glass tubing about eight or nine inches long, the unattached end having a 15° bevel. The flask is placed upside down and held in place by a clamp on an iron stand (B). Some distance below the flask, a stender (C) filled with water and containing the tissues is placed. The flask is lowered until the bottom of the bevel of the glass tube extends 1/16 inch below the water level in the stender. Leading from the stender, a piece of capillary tubing, bent at a 50° angle 1½ inches from one end and at the other end drawn to half the original bore, is held in place by a piece of plastic clay moulded over the edges of the stender (D). If the long arm of the siphon is placed in almost a vertical position, the liquid will drain off the tissues at a very rapid rate, but the greater the tendency toward a horizontal position, the more noticeable will be the decrease in the number of drops siphoned per minute. However, the siphon will not work unless the overflow end is placed on a lower level than the suction end, and the suction end must be placed near the bottom of the stender.

As the siphon drains the liquid off the tissues, the level of the mixture of alcohol and water falls below

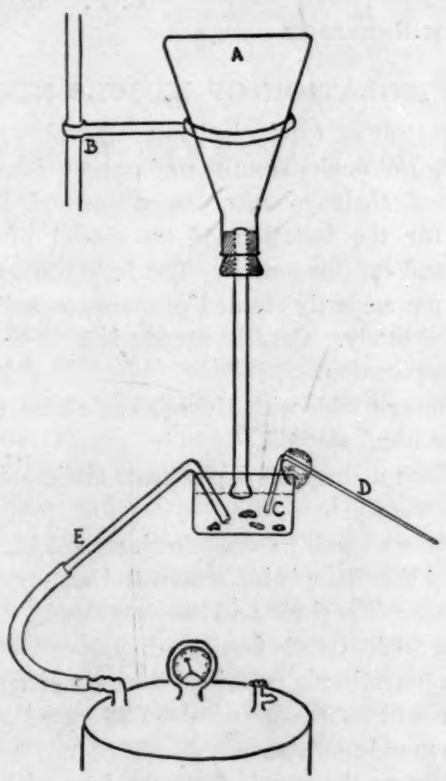

FIG. 1

[2] G. J. Leuck, R. P. Perkins and F. C. Whitmore, *Jour. Am. Chem. Soc.*, 51: 1834, 1929.

the lower edge of the bevel, which causes air to be drawn back into the flask and more alcohol is then introduced into the stender from the flask; when the level of the liquid in the stender reaches the level of the bevel, the flask of alcohol will no longer empty itself.

A provision has been made to insure the proper mixing of the alcohol and water in the stender. An L-shaped piece of glass tubing is connected to a rubber hose which runs to any source of compressed air (E). In the apparatus shown above, an old gas tank, provided with a pressure gauge, inlet and outlet stopcocks, was filled with air by an ordinary automobile air pump, so as to yield a pressure of 20 to 25 pounds. The rubber tube was attached to the outlet and the stopcock was regulated so that there would be 50 to 75 bubbles of air per minute when placed in water. Care must be taken to place the outlet of the L-shaped tube near the bottom of the stender; otherwise the alcohol and water will not mix properly.

If the siphon is regulated at eight drops per minute, the tissue (if placed in 20 cc of water to begin with) will be in approximately 97 per cent. alcohol after twenty-four hours, and is then ready to be transferred into absolute alcohol for 3 to 6 hours, before being placed in the clearing agent.

The apparatus is effective and easy to construct. One should be available on each table of the technique laboratory.

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THE ISOLATION OF MUSCLE NUCLEI

As a matter of cytological interest and as a means of freeing the nuclei from the cytoplasm for chemical analysis of their constituents, a method has been devised for the isolation of the nuclei of smooth, striated and cardiac muscle. The following procedure yields a permanently stained preparation suitable for cytological study. Cardiac muscle is suggested as an initial preparation.

(1) Smear a slide with Mayer's egg albumen according to the usual method.

(2) Place a drop of 5 per cent. citric acid in the center of the slide.

(3) Place a small piece of fresh muscle in the drop. Gradually the tissue is infiltrated and assumes a translucent state. The citric acid becomes cloudy, due to the released nuclei. Gentle teasing will hasten the reaction. If the preparation is examined microscopically at this time, one will note large numbers of nuclei streaming from the muscle substance.

(4) Remove the muscle from the drop with forceps. The resulting preparation will contain nuclei free of cytoplasm.

(5) Allow the preparation to dry almost completely. The slide is then placed in 95 per cent. ethyl alcohol which coagulates the albumen, thus holding the nuclei in place.

(6) Rinse in several changes of tap water, followed by distilled water.

(7) Transfer to Mayer's hemalum until the nuclei are stained.

(8) Wash in tap water until blue.

(9) Counterstain in eosin.

(10) Dehydrate, clear and mount.

The technique is comparatively simple. The only possible cause of failure is the washing off of the preparation in step five. Perhaps a repeated attempt may be necessary for determination of the optimum time for drying the slide before placing in the 95 per cent. alcohol.

The resulting preparation shows an abundance of nuclei stained blue on a quite homogeneous eosin-stained background. The latter is presumed to consist of egg albumen and any muscle fraction soluble in the citric acid. Although the technique is applicable to a number of other body tissues, somewhat inferior results are obtained.

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BOOKS RECEIVED

BUTLER, LORINE L. Birds Around the Year. Pp. xi + 242. 8 plates. Appleton-Century. $2.00.

Culture Methods for Invertebrate Animals. A Compendium prepared cooperatively by American zoologists under the direction of a committee from Section F of the American Association for the Advancement of Science. PAUL S. GALTSOFF, FRANK E. LUTZ, PAUL S. WELCH and JAMES G. NEEDHAM, Editors. Pp. xxxii + 590. 84 figures. Comstock. $4.00.

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